Environmental Modeling and Decision Support Tools for Large, Multi−Scale Problems in Estuary Management

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Public Comments

No public comments were received for this proposal.

Collaboration Panel Review

Proposal Title

#0158: Environmental Modeling and Decision Support Tools for Large, Multi−Scale Problems in Estuary Management

Final Panel Rating
adequate

Collaboration Panel (Primary) Review

Collaboration:

Will the results of the collaborative effort be greater than the sum of its parts? Is it clear why the subprojects are part of a larger collaborative proposal rather than several independent smaller ones?

above average
The subtasks within each task are prescribed sequentially, and the dependance of tasks 5 and 6 on Task 4 is specifically referenced (page 13). The final sub−task, 6.4, is the release of documentation on the use of the updated model REALM which is the product of all previous sub−tasks. Each sub−task involves personnel from the two principal applicant organisations (LBNL and DWR).

Interdependence And Integration:

Does the proposal have an example that clearly articulates the conceptual model of each subproject and how they link together as a whole? Are the boundaries of the study plans focused and cohesive, yet well delineated? Is there a plan for potential differences in the stages of subproject completion times? Are there clear plans for analyses and interpretations which seek to identify and quantify relationships among the data collected in various subprojects rather than separate analyses for each subproject?

above average
No conceptual model is presented. The Plan of Work (Section 2.2, pages 7−18) and the Tasks form clearly delineate the nature, sequence and inter−relationship of the tasks, but does

not detail how collaboration will occur. The proposal would have benefitted from having a graphical display of the timeline – the text only identifies the duration of each sub−task. The sequence of the sub−task poroducts is well conceived, with the final deliverable occurring as a result of the output from the previous work.

Project Management:

Is it clear who will be performing management tasks and administration of the project? Are there resources set aside for project management and time given for investigators to collaborate? Is there a process for making decisions during the course of the project? Are there acknowledgments of potential barriers to collaboration and explanations of how team members will overcome barriers particular to their institutions?

inadequate
The Lead Investigator is identified, but it is not clear who will lead the subtasks. There is no discussion of how to address problems that occur during the course of the project or the communication that would occur between staffs on concurrent subtasks.

Team Composition:

Does the lead principal investigator have successful management history and experience leading collaborative teams? Is it clear that all key personnel are committed to making significant contributions to the project? Do team members have complementary skills?

above average
The Lead Investigator apparently has the proper background in leading collaborative efforts; the resume indicates such experience but does not specify the collaborator entities. The time allocation for key personnel is substantial. The expertise of the various team members seems to match that required by the various sub−tasks, and appear to be complementarity on each sub−task. The listing of sub−task participants in the Plan of Work (Section 2.2, pages 7−18) is frequently inconsistent with the Tasks form and Budget form listings of sub−task participants.

Communication Of Results:

Is there a clear plan for comprehensive and cohesive reporting of project progress to the CALFED community?

inadequate
The text in Section 3.2 (Dissemination of Work, Page 20) indicates all work prepared from the proposal will be in the public domain and will be reported in various outlets, but details of how theis will occur are not given. Intermediate progress reports are referenced but are not identified as products elsewhere in the proposal. The text indicates that DWR is required to report annually to the SWRCB on its model development progress, so it is implied that work in this proposal would be reported. Proposal results are to be presented at the IEP and CWEMF workshops, and classroom presentations will be made by DWR staff and their collaborators. There are no dedicated tasks with associated funding to accomplish the communication of results. It is unclear how the presentation of results in peer−reviewed publications would occur in the absence of funding after the termination of this proposal.

Additional Comments:

Collaboration Panel (Discussion) Review

Primary and secondary reviewers initially disagreed on the final overall rating; for example, the plan for communication of results was debated: one judged it inadequate (felt the applicants overpromised what couldn't be delivered, whereas the other initially judged it superior (took the words for their worth). Secondary reviewer, after athorough, clarifying panel discussion, adjusted his rating to adequate.

Technical Synthesis Panel Review

Proposal Title

#0158: Environmental Modeling and Decision Support Tools for Large, Multi−Scale Problems in Estuary Management

Final Panel Rating
adequate

Technical Synthesis Panel (Primary) Review

TSP Primary Reviewer's Evaluation Summary And Rating:

The proposal aims to provide tools, rather then to develop scientific understanding, by adding functionality to REALM (River, Estuary and Land Model), an on−going collaborative effort between DWR and LBNL. In particular, (1) particle movement with behavior will be added to allow description of local and large−scale migration of important species, and (2) the capability to model the evolution of shorelines as a result of tidal and seasonal wetting and drying will be added. While worthy goals, their importance relative to overall CALFED priorities are not justified. Other modeling approaches or efforts are not compared and contrasted, and validation and uncertainties are not duly considered.

Additional Comments:

The objective of this proposal is to develop a series of improvements to the REALM model. Meeting this goal is a matter of efficient programming and timely data assembly. The proposal is essentially a software development program, written too much like an internal document. It presumes the reader is familiar with REALM, and that the value of the proposed improvements is self−evident. Thus, a reviewer either had to be familiar with REALM to verify that the proposed

upgrades were both practical and of high priority, or take it on faith. The developmental approach is generally well developed. One essential step is particle tracking (using modified Kalman filters and parallel processing to enable scaling up to the entire Delta). The other essential step is the simulation of wetting and drying cycles to enable Cartewsian cell cutoff in order to estimate changes in the shoreline boundaries of shallow embayments and tidal sloughs. Less major steps include algorithms for tangent linearization for shallow water, algorithms for mid−course model steering and for data assimilation, and optimization routines. Scaling up using parallel processing and Kalman filters is a useful technique for accommodating size of the model with reasonable computing speed. Taken together, the planned upgrades represent a development that can support model experimentation on a Delta−wide basis. Major questions concern model validation and application, neither of which is addressed adequately in the proposal. The authors point out that the original REALM is still not finished, and is not available as evidence supporting the feasibility of the proposed upgrades. The integration of field data into the revised REALM model would guarantee some degree of accurate prediction, as would the mid−course steering algorithms. However, adequate validation is likely to require some form of satisfactory predictive or postdictive simulation of events, none of which are specified. Moreover, the data seem to be lacking for a quantitative validation, e.g., how will neutrally buoyant particles and fish distributions are be tracked? Some serious monitoring studies will have to be designed or (if they exist) coordinated with a validation effort, and this aspect is not well developed in the proposal.

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While worthy goals, their importance relative to overall CALFED priorities are not justified. Other modeling approaches or efforts are not compared and contrasted, and validation and uncertainties are not duly considered.

Technical Synthesis Panel (Discussion) Review

TSP Observations, Findings And Recommendations:

The technical reviewers and the panel agreed that the research team is capable of performing the proposed algorithm development. However, the proposed work is essentially software development and lacks a scientific context. Further, the proposal lacked sufficient content regarding how the enhancements to the model would be validated.

Technical Review #1

proposal title: Environmental Modeling and Decision Support Tools for Large, Multi−Scale Problems in Estuary Management

Review Form

Goals

Are the goals, objectives and hypotheses clearly stated and internally consistent? Is the idea timely and important?

Comments	**The goals, objectives, and hypotheses are all clearly stated and consistent. This problems that this project will address are extremely well laid−out. There are two ideas pursued here: 1. to augment an existing modeling framework with sophisticated and efficient capabilities to simulate both particle transport (with some biological attributes) and changing estuarial morphology, and 2. to incorporate decision support capabilities. The overall idea is to provide a tool for decision−makers that will facillitate decisions on water management with respect to effects on multiple concerns, including water quality, supply, and fish. This idea seems very timely and important.**
Rating	**excellent**

Justification

Is the study justified relative to existing knowledge? Is a conceptual model clearly stated in the proposal and does it explain the underlying basis for the proposed work? Is the selection of research, pilot or demonstration project, or a full−scale implementation project justified?

Comments	**The study is justified by existing knowledge (or, lack thereof). First, the concepts that will be**

	incorporated are available in disciplines other than water modeling and would be useful in this field. Second, there is a clear need for reliable modeling in the SF Bay−Delta. Currently, water quality models that do exist are under−utilized and do not carry much currency (for a variety of both understandable and unexplained reasons). A research project of this quality would be very valuable. The conceptual approach of the project and its basis are both well laid out in the proposal. The level of effort and scope are thoughtfully designed and well considered.
Rating	**excellent**

Approach

Is the approach well designed and appropriate for meeting the objectives of the project? Is the approach feasible? Are results likely to add to the base of knowledge? Is the project likely to generate novel information, methodology, or approaches? Will the information ultimately be useful to decision makers?

Comments	**Yes, the approach to the problem is well designed and appropriate for the obljectives. Steps along the way to completion are carefully laid out and clearly described. Each step seems to achieve tangible results and provides a building block for the rest of the development. These are new approaches to modeling (at least for this part of the world) and these innovations are likely to be useful to decision makers and the general scientific community, providing that the REALM platform and all the components remain open−source, well−documented, well−supported, and available.**
Rating	**excellent**

Feasibility

Is the approach fully documented and technically feasible? What is the likelihood of success? Is the scale of the project consistent with the objectives and within the grasp of authors?

Comments	**Approach is fully documented and technically feasible. The authors are all capable of grasping the scale of the project and, in fact, have limited the scope to a reasonable effort as a stage in development of a larger modeling system.** **To be accepted, the system must have some kind of simple interface for scientists not familiar with these types of models, but it must be sophisticated and accessible for those that are. DWR (one of the proponents) has done well in these respects with its models. DSM2 is a good example. A concern of mine is that the Bay−Delta scientific community, in general, has not supported application of these models. We have not seen water quality models incorporated into studies as regularly as they could (or should) be. So, an effort like this might be academic in the current Bay−Delta scientific environment. One only hopes that the local scientific community will be more interested in encouraging the use of these models in Bay−Delta studies of all kinds.**
Rating	**very good**

Monitoring

If applicable, is monitoring appropriately designed (pre−post comparisons; treatment−control comparisons)? Are there plans to interpret monitoring data or otherwise develop information?

Comments	**Monitoring is not given much weight in this proposal. I believe that there is some mention of using GIS daa to evaluate results and there is a suggestion of coordination with DWR monitoring studies. Certainly, extensive monitoring will be done for the data assimilation component. The proposal mentions validating particle transport in the Yolo Bypass, Delta, and Suisun Marsh but this seems qualitiative.** **The data seem to be lacking for a quantitative validation and this is a drawback of the proposal. I don't see how the model components will be validated**

	with real data. Probably, morphology and water surface elevations can be tracked with reasonable success. But how do we know that neutrally buoyant particles and fish distributions (or any water quality parameters) are tracked well? Some serious monitoring studies will have to be designed or (if they exist) coordinated with and this aspect of model validation is not not well developed in the proposal. Still, an unvalidated model is useful as a conceptual tool. It can guide future monitoring studies and eventually lead to a validated product.
Rating	good

Products

Are products of value likely from the project? Are contributions to larger data management systems relevant and considered? Are interpretive (or interpretable) outcomes likely from the project?

Comments	The project will produce a sophisticated modeling system for use in decision support. Larger data management systems are well considered by the authors. The model, until properly validated, will be mostly conceptual in utility but it will provide interpretive and interpretable outcomes. Development of project components will be of use and interest to the Bay−Delta scientific community and to decision makers. Certainly, they will be relevant to the promotion of environmental models as useful support tools in all aspects of decision making and data collection.
Rating	very good

Additional Comments

Comments	None.

Capabilities

What is the track record of authors in terms of past performance? Is the project team qualified to efficiently and effectively implement the proposed project? Do they have available the infrastructure and other aspects of support necessary to accomplish the project?

Comments	**The team looks to be qualified, efficient, and effective. They understand the problem well and have the infrastructure for support to complete the project. The project appears to be a part of a larger effort by two respected scientific entities, DWR and Lawrence Livermore. I don't know the Livermore contributors, but the DWR contirbutors have good track records and are respected in the modeling community.**
Rating	**excellent**

Budget

Is the budget reasonable and adequate for the work proposed?

Comments	**The budget, though large and a little confusing to me, seems reasonable and adequate. I am assuming that DWR efforts are listed under "overhead" for some reason. I think that this is a large effort and it needs meticulous work, so I'm not surprised at a budget that would be somewhat at the high end.**
Rating	**very good**

Overall

Provide a brief explanation of your summary rating.

Comments	**I like this proposal. I think that it is well laid out and that its goals are achievable. The project will bring new tools and a new perspective to the Bay−Delta modeling community and, hopefully, to scientists and decision−makers. Although the goals are well−defined, the product will be of general use mostly in a**

	concepual way (i.e. how might a fish population be distributed under certain flow conditions) until some kind of quantitative validation is designed and carried out. The project advances science and decision−making in the Bay−Delta area.
Rating	**excellent**

Technical Review #2

proposal title: Environmental Modeling and Decision Support Tools for Large, Multi−Scale Problems in Estuary Management

Review Form

Goals

Are the goals, objectives and hypotheses clearly stated and internally consistent? Is the idea timely and important?

Comments	**The objective of this proposal is to develop a series of improvements to the REALM model. Collectively, these improvements are intended to permit REALM to receive the highest priority upgrades specified in a survey of potential users: to establish the dynamics of wetting and drying on shoreline boundaries and to establish particle tracking capacities to the model to simulate estuary−wide hydrodynamics. Since there are no more precise scientific objectives, the goal simply is to complete the REALM upgrades. Meeting this goal is a matter of efficient programing and timely data assembly, and having no major unanticipated problems. The proposal is thoroughly specified in detail, so presuming that REALM is going well, the experience of the DWR−LBNL team suggests that the goal is feasible. Evaluating goals in terms of more traditional scientific protocols is impossible, since this proposal is essentially a software development program which presumably will be useful for unspecified future projects in a future work plan.**
Rating	**good**

Justification

Is the study justified relative to existing knowledge? Is a conceptual model clearly stated in the proposal and does it explain the underlying basis for the proposed work? Is the selection of research, pilot or demonstration project, or a full−scale implementation project justified?

Comments	**Although the goal may be practical, it is extremely difficult to know how much confidence to have in this statement. The proposal is written too much like an internal document. It presumes the reader is familiar with REALM, and that the value of the proposed improvements is self−evident. Thus, a reviewer either had to be familiar with REALM enough to verify that the proposed upgrades were both practical and of the highest priority, or take it completely on faith. I do not think requiring such a leap of faith is justified here. This aspect is one of the biggest weaknesses of the proposal.**
Rating	**fair**

Approach

Is the approach well designed and appropriate for meeting the objectives of the project? Is the approach feasible? Are results likely to add to the base of knowledge? Is the project likely to generate novel information, methodology, or approaches? Will the information ultimately be useful to decision makers?

Comments	**The developmental approach is generally well developed. One essential step is particle tracking (using modified Kalman filters and parallel processing to enable scaling up to the entire Delta). The other essential step is the simulation of wetting and drying cycles to enable Cartewsian cell cutoff in order to estimate changes in the shoreline boundaries of shallow embayments and tidal sloughs. Less major steps include algorithms for tangent linearization for shallow water, algorithms for mid−course model steering and for data assimilation, and optimization routines. Scaling up using parallel processing and Kalman filters is a useful technique for accommodating size of the model with reasonable computing speed. Taken together, the planned upgrades represent a development that can support model experimentation on a**

	Delta−wide basis. If accomplished, such a step would represent a major step forward to better delta resource management through improved impact assessment, provide that the numerical nature of the model does not require so many adjustments as to undercut its contribution to causal understanding.
Rating	**very good**

Feasibility

Is the approach fully documented and technically feasible? What is the likelihood of success? Is the scale of the project consistent with the objectives and within the grasp of authors?

Comments	**See field 2. The project is tightly integrated on a three−year scale, with little room for major delays. However, my best guess is that the Kalman filter modifications and other necessary algorithms can be written and tested against data within the time span of the proposal, My major questions concern model validation and application, neither of which is addressed adequately in the proposal. The authors point out that the original REALM is still not finished, and is not available as evidence supporting the feasibility of the proposed upgrades. The integration of field data into the revised REALM model should guarantee some degree of accurate prediction, as would the mid−course steering algorithms. However, adequate validation is likely to require some form of satisfactory predictive or postdictive simulation of events, none of which are specified. We are left to wonder what the priority real−world problems are that will be tackled by the upgraded REALM model. This is particularly curious, given the emerging importance of the Yolo Bypass, its selection as a target subsystem in this propopsal, and the participation of Ted Sommer in the project – yet with little in the way of a defined role for him. One would think that Sommer's work on splittail would be an obvious target for this proposal.**

Rating	**good**

Monitoring

If applicable, is monitoring appropriately designed (pre−post comparisons; treatment−control comparisons)? Are there plans to interpret monitoring data or otherwise develop information?

Comments	**There is no monitoring in this proposal.**
Rating	**not applicable**

Products

Are products of value likely from the project? Are contributions to larger data management systems relevant and considered? Are interpretive (or interpretable) outcomes likely from the project?

Comments	**The upgraded REALM is the major product of this proposal. If it is successful, REALM may provide a protocol for other large scale systems, although Kalman filters and parallel processing already are in use for such systems as the Chesapeake Bay. More likely, REALM could be an adequate model and potentially important tool for Bay−Delta system management However, the absence of more explicit target problems and validation is a weakness that could have easily be addressed – and should be.**
Rating	**fair**

Additional Comments

Comments	**none**

Capabilities

What is the track record of authors in terms of past performance? Is the project team qualified to efficiently and effectively implement the proposed project? Do they have available the infrastructure and other aspects of support necessary to accomplish the project?

Comments	**This is a highly capable technical staff, with experience in all necessary areas, working in a lab setting known for past contributions to the natural sciences.**
Rating	**excellent**

Budget

Is the budget reasonable and adequate for the work proposed?

Comments	**The budget request is large (1.26 million) and almost all tied into salaries and benefits. The real budget is even larger, given the in−kind contribution of DWR personnel to the project. However, the details are such that the budget is reasonable, given the number of investigators involved, provided that the model is completed and on schedule. Whether the time schedule can be met remains the single most problematic aspect of the budget side of the proposal, since the model must be completed to be sure the major deliverable will be available.**
Rating	**very good**

Overall

Provide a brief explanation of your summary rating.

Comments	**The potential of the REALM model is very good and the ability of the research team to complete it is satisfactory, yet there is too much unspecified, from the current status and prognosis of REALM, to plans for validation and actual use, for this proposal to be any more than another phase of model development without a better feeling for its ultimate use in the Delta.**
Rating	**good**

Technical Review #3

proposal title: Environmental Modeling and Decision Support Tools for Large, Multi−Scale Problems in Estuary Management

Review Form

Goals

Are the goals, objectives and hypotheses clearly stated and internally consistent? Is the idea timely and important?

Comments	**The goals to add significant environmental modeling and decision support functionality to REAL, the objectives (to develop models of particle or specy tracking and of coastline wetting/drying and to provide suppor tools/software for modeling), and the hypotheses are clearly stated and internally consistent.** **The idea is timely and important to the environmental modeling in Bay−Delta region.**
Rating	**very good**

Justification

Is the study justified relative to existing knowledge? Is a conceptual model clearly stated in the proposal and does it explain the underlying basis for the proposed work? Is the selection of research, pilot or demonstration project, or a full−scale implementation project justified?

Comments	**The study is justified relative to existing knowledge of large, multi−scale modeling.** **The conceptual model is stated in the proposal and it explains the underlying (computational and hydrological) basis for the proposed work.** **The selection of research project with the full−scale**

	implementation is justified.
Rating	**very good**

Approach

Is the approach well designed and appropriate for meeting the objectives of the project? Is the approach feasible? Are results likely to add to the base of knowledge? Is the project likely to generate novel information, methodology, or approaches? Will the information ultimately be useful to decision makers?

Comments	**The approach is well designed and appropriate for meeting the objectives of the project and is feasible.** **The results are likely to add to the base of knowledge.** **The project is likely to generate novel information, but limited to existing methodology or approaches.** **The information will ultimately be useful to decision makers, directly or indirectly.**
Rating	**good**

Feasibility

Is the approach fully documented and technically feasible? What is the likelihood of success? Is the scale of the project consistent with the objectives and within the grasp of authors?

Comments	**The approach is documented and technically feasible. However, the hydrodanymics of caostline wetting/drying and its modeling approach is not full explained.** **The likelihood of success is high.** **The scale of the project is consistent with the objectives and within the grasp of authors.**

Rating	**good**

Monitoring

If applicable, is monitoring appropriately designed (pre−post comparisons; treatment−control comparisons)? Are there plans to interpret monitoring data or otherwise develop information?

Comments	**Monitoring is not proposed. There are plans to interpret monitoring/observation data for applications in Yolo Bypass habital area and other areas.**
Rating	**good**

Products

Are products of value likely from the project? Are contributions to larger data management systems relevant and considered? Are interpretive (or interpretable) outcomes likely from the project?

Comments	**Products of value (software/tools mainly) are very likely from the project.** **Contributions to larger data management systems are relevant and considered, but integration to theses systems are not stated clearly.** **Interpretive (or interpretable) outcomes from software/tools and their applications are likely from the project.**
Rating	**good**

Additional Comments

Comments	**Would be helpful if the leading role for each primary staff is indicated in tasks form.**

Capabilities

What is the track record of authors in terms of past performance? Is the project team qualified to efficiently and effectively implement the proposed project? Do they have available the infrastructure and other aspects of support necessary to accomplish the project?

Comments	**The track record of authors in terms of past performance is excellence and impressive in computing and modeling.** **The project team is qualified to efficiently and effectively implement the proposed project.** **They have available the infrastructure (mainly computers) and other aspects of support necessary to accomplish the project.**
Rating	**good**

Budget

Is the budget reasonable and adequate for the work proposed?

Comments	**The budget is reasonable and adequate for the work proposed. It is unclear the time (hours) devoted to each task for each individual.**
Rating	**fair**

Overall

Provide a brief explanation of your summary rating.

Comments	**The proposal's strengths: (1) improvement of the capability to accurately model particle moevement and the evolution of coastline; and (2) integration of the imrpovement to REALM.** **The weaknesses are (1) no detailed and quantitative discussions on modeling accurtity impacts on particle**

	movement and coastline evolution with daily or seaonal variations of hydrodynamics in Bay−Delta area; and (2) presenting a list of software/tools instad of focusing one integrated package addressing/solving one particularly important issue.
Rating	**good**